SCIENCE.

FRIDAY, JANUARY 30, 1885.

COMMENT AND CRITICISM.

THE LACK of truly demonstrative evidence, in the solution of certain geological problems that have been regarded as settled for years before they are overthrown, finds new illustration in the remarkable results lately announced by the geological survey of Great Britain, which form the subject of a paper by one of the contributors to our paper this week. The conclusion, that now seems to be erroneous, rested on what may be called the argument from continuity of deposit. The same argument, involving the same error, was used by Werner nearly a century ago to prove the aqueous origin of his 'floetz-trap.' These old lava-flows apparently formed part of a continuous series with the underlying sedimentary strata, and hence were thought to be, like the latter, of sedimentary origin; and this conclusion held until an abrupt contact-line, that had previously escaped notice, was found between the dissimilar formations. Precisely the same reasoning has been employed in recent years to support the aqueous origin of the old lavas in the Palisades of the Hudson; but the method of disproving the error in such a case is now too well known, and in this example is too easily applied, to allow any general acceptance of so visible a mistake. In the same way, the essential element in the observations which Murchison and Geikie considered conclusive as establishing the Silurian age of certain Highland schists and gneisses was the continuity of the series, without break by unconformity or dislocation, from the underlying fossiliferous beds to the overlying crystalline members; and, on the strength of their report to this effect, the Silurian age of the now crystalline masses has been for years accepted by many geologists.

Now, it appears that these early observations were too hasty. Examination by more sceptical observers, and recent deliberate official studies mapped on the ideal scale of six inches to a mile, discover a most peculiar discontinuity in the form of a nearly horizontal surface of dislocation, across which the overlying mass has been driven actually for miles from its normal inferior position. Whatever possibilities may be discovered elsewhere, the paleozoic date for the metamorphism of the Sutherland crystalline series must now be regarded as incorrect, and the origin of their crystalline texture must be set back into earlier ages. The character of the dislocations thus revealed is as important as the disproof they afford of a serious error; and the inverted attitude that has been observed elsewhere between fossiliferous and crystalline beds will be examined over again in the light of these fruitful discoveries. These overriding Scotch gneisses may thus prove to be the connecting-link between the well-established alpine inversions that lay the fundamental gneiss on mesozoic limestone, as on the northern cliffs of the Jungfrau, and the still unsolved mystery in Norway, where crystalline schists seem to overlie the fossiliferous paleozoic sediments across wide areas, and thus give an abnormal character to the structure of the mountains, as shown in Törnebohm's section of the peninsula.

Then there is the extraordinary measure of ten miles for the horizontal displacement that is accountable for the whole difficulty in the Highlands; and along with this goes the occurrence of a number of (so-called) 'reversed faults,' in which the uplifted member has been thrust up an inclined plane. All of this is strong in evidence of the modern view that disordered mountain structures are characterized less by the gain of height than by the loss of breadth that they have suffered. The almost

incredible transgression of an older mass upon a newer one, now reported, has few parallels, unless one may be found in the famous overturning of the Windgällen Alps, studied out by Escher von der Linth, and confirmed by Heim. In the face of such an example, so utterly beyond explanation without the aid of irresistible lateral compression, we feel that the contractional hypothesis gains new support; and against the English school of physical geologists, who claim to show its insufficiency, the conclusion of Heim may be now quoted with new force: more may be learned of the earth's structure from critical observations on its surface than from calculations founded on physical assumptions concerning its interior. Besides these extraordinary facts of motion, the production of chemical changes during the mechanical stresses and distresses of the Highlands is hardly less remarkable. Sandstone passes into gneiss, and gneiss acquires schistosity, in a new direction in obedience to distorting forces. All this is beautifully confirmatory of Lehmann's conclusions from his researches in Germany during the past few years. Mechanical metamorphism thus gains the support of a series of facts that chemical metamorphism can ill afford to lose.

THE article on this subject, contributed to the present number of *Science* by a well-known writer on these matters, contains certain statements to which exception may be taken. The questions raised with regard to the New-England rocks here referred to certainly cannot be considered 'settled' in the manner implied by our contributor, as was very evident at the Montreal meeting of the British association last summer; and the metamorphic origin of certain alpine rocks is not yet generally abandoned. As to 'regional metamorphism,' the revelations of recent detailed and minute studies in the field are not always such as to disprove it, but rather to attribute the metamorphic changes, where they occur, to mechanical instead of simply to chemical action; and, when disturbed and distorted rocks are found in altered mineralogical conditions over considerable areas, 'regional metamorphism' does not seem to be a misleading or confusing term to apply to them. Finally, the implication that Mr. Geikie makes insufficient reference to the results of his predecessors is certainly unwarranted. He states sufficiently that other observers have preceded him in the views he has now come to hold, and promises that they shall be duly mentioned in the detailed report which is to follow the present brief and preliminary publication. His outspoken frankness in admitting his previous error leaves nothing to be desired, and sets an example worthy of imitation.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Trowbridge's Physics.

IN a recent number of *Science* will be found a criticism of Professor Trowbridge's 'New physics.' Those who have carefully read the work alluded to have doubtless found errors here and there, which would not appear in a second edition; but no one can fail to recognize a master mind in the organization of this new method of teaching natural science. The allegation that Professor Trowbridge has misstated some of the fundamental laws of mechanics is not sustained by a closer examination.

It is hardly necessary to point out that the formulae for the ballistic pendulum become perfectly intelligible if we understand by the first h, not the maximum height attained, but the observed distance through which the pendulum is acted upon by the force F, whose average value is thus determined 'without involving the element of time;' that the laws for the lever, which caused the critic even more surprise, are perfectly correct, when, as in the case in point, angular acceleration is considered, since the work spent upon equal masses, like their moment of inertia, is in this case proportional, as stated, to the square of the distance from the fulcrum; that it is indifferent, in the experiment, whether we find the length, or the radius of gyration, of the equivalent simple pendulum, since the two are identical; and that force is constant over the concentric spherical equipotential surfaces in question. The last two statements, therefore, as made by the author, need only to be restricted.

Such oversights as the critic is easily able to detect are not insidious, like some of those which have crept into many modern text-books. The underlying principles are brought out by the course of experiments in their clearest light; and therefore the work must be regarded by teachers as a safe and trustworthy guide.

It may be added that the experiments considered so difficult by the critic have already been employed with success in an elementary course, and are undoubtedly in place in any work whose object is to

elevate, rather than cater to, the present standard of physical instruction in the schools. W.

[The 'master mind' was distinctly recognized, and its presence cheerfully acknowledged, in the review to which the above refers. The reviewer heartily joins his critic in his desire for a 'closer examination' to determine the justice of the comments made. Such an examination will unquestionably show that every criticism made in the review is well founded. It will be generally admitted that an explanation which needs explaining is not extremely satisfactory. The points under discussion are such as are not usually considered in books with which the teacher is likely to be familiar; and erroneous and confusing statements will generally be accepted, although not understood. The result must be disheartening, if not disastrous. It seems wiser, therefore, to warn him to be on the lookout for errors which have not been eliminated from this first edition, but which are not likely to be found in a second. And this is especially true of a book which contains as many really good and original things as the 'New physics,' and which carries the weight which naturally and necessarily goes with any thing Professor Trowbridge writes. — REV.]

The earthquake of Jan. 2.

Supposing that reasonably exact determinations of the time and character of seismic phenomena are useful, I send the following note on the shock of Jan. 2 at Washington.

I recognized the character of the shock at the instant of its occurrence, and timed it. On the following day, comparing my watch with one set to the standard (not local) time adopted for this city, I found the shock occurred at 9 h. 16 m. P.M., civil time, to which the correction to the Washington meridian is to be applied. My residence is close to Ascension church, on the highest land away from the boundaries of the city: the grade is ninety-two feet above mean level of the river, and two feet higher than the base of the capitol. I was in the third-story back room, facing east into the back-yard, and south into an alley. The house is of brick, and above the middle of the second story is isolated. The shock was a distinct and very heavy and sudden jar, not accompanied by noise, unless by a slight rattling of the windows, and lasted less than a second. The sensation was as if a very heavy body had struck the earth, yet also as if the jar were partly upward rather than downward. There was no second shock within fifteen minutes, although I saw a paragraph in the daily press to the effect that one individual alleges that he felt a second shock about 11 P.M. at Alexandria, Va. W. H. DALL.

Itinerant science-teachers.

In *Nature* for Dec. 25, 1884, there is described an 'itinerant method of science-teaching,' which "has been carried out on a large scale and with the most gratifying success by the school boards of Birmingham and Liverpool." A science demonstrator is appointed for a number of schools; and he is provided with apparatus, which is conveyed from school to school in a handcart 'by a strong youth.'

"The system," it is said, "fairly meets the objections which have been urged against the introduction of science-teaching, on the grounds of want of qualified teachers, want of time [to prepare for the lessons], and cost of apparatus. It also secures systematic and continuous teaching throughout the school-year. The teaching is practical, and every fact or law is demonstrated experimentally."

Would it not be well to try a similar plan here?

J. R. W.

[It would answer in large centres, but would be limited in its application to places where it might be said to be least needed. — ED.]

The voice of serpents.

The text-books upon zoölogy represent that the vocal apparatus of serpents is very scantily developed, only enough to enable some of these creatures to hiss. A fact lately brought to my attention by Mr. George W. Leitch of Ryegate, Vt., is worthy of mention, and may lead herpetologists to search more carefully for the vocal apparatus of serpents. Mr. Leitch was stationed for several years at Manepy, Ceylon, as a missionary of the American board of commissioners for foreign missions. One day a serpent entered an apartment containing lumber, and it was deemed best to kill him. It became very angry, and made a loud noise, which Mr. Leitch says reminded him of the bellowing of a bull two years old. Perhaps others may know of instances in which these creatures make loud noises. This animal was of an uncommon variety, and was not preserved. It was of considerable size, say, seven or eight feet in length.

C. H. HITCHCOCK.

Hanover, N.H., Jan. 16.

The incandescent light on steamers.

In No. 102 of *Science*, in the article on 'Recent advances in electrical science,' Professor Trowbridge makes the statement that the Fall-River line took the initiative in adopting the incandescent system. This is certainly a mistake, as I myself saw it in full operation on the Virginia, of the Bay line (running between Baltimore and Norfolk), in the autumn of 1882, about a year before the Pilgrim was launched. Whether the Bay line was the first to adopt it or not, I do not know.

EVERETT HAYDEN.

U.S. geol. surv., Washington, D.C., Jan. 19.

Rainfall and crops.

Professor Snow's statement (*Science*, v. p. 13), that an annual rainfall of eighteen inches is entirely inadequate to maintain successful agriculture, is, I suppose, meant to apply only to Kansas, and, with that limitation, may be correct. In California, and especially in this portion of it, our experience is very different. Properly distributed, a rainfall of ten inches is ample to mature the cereals, and excellent crops are frequently raised with less. In the season of 1881–82 this place had 4.89 inches of rain, and there was an almost complete failure of crops, except on irrigated land. In 1882–83 there were 5.86 inches; and the distribution could hardly have been worse, almost all the rain falling after the 26th of March. Even under such circumstances there was some production on dry land, and the opinion was general that the crops would have been fair if the same amount of rain had come at the proper times. Last year the rainfall was almost unprecedented, reaching 18.32 inches. It was altogether too much. The crops were good, but they would have been far better if the last inch or two had been omitted. Of course, under different conditions of soil and climate,

eighteen inches may be too little; but here an assured minimum of ten, or even eight inches, would rob farming of all its terrors.

S. E. MOFFETT.

Kingsburg, Fresno county, Cal., Jan. 13.

The use of slips in scientific correspondence

To find that different workers have independently reached the same conclusions, or that they have adopted the same expedients to facilitate their work, is an evidence of the justness of the conclusions, or the excellence of the expedients. This reflection is suggested by the perusal of Professor Wilder's note of above title in *Science* of 16th inst , p. 44. At the time (May 15, 1867) when Professor Wilder announced to the Boston society of natural history his use of slip-notes, I remarked that I had used slips in a similar manner; and now I can say that my principal colleague in the editorship of *Psyche*, Dr. George Dimmock, has for a long time exchanged with me, and probably with others, correspondence-slips for purposes similar to those described by Professor Wilder, and that I have used the card-catalogue system with profit for all the purposes mentioned by Professor Wilder and for others.

The essential features of slip-systems for filing away notes are the use of a standard or uniform size of paper for all purposes, and the entry of but one subject on a slip. After many and various experiments in the attempt to combine these features with others which are desirable, I have found the following arrangement the most convenient for all files which are not kept as card-catalogues purely. I procure thin manila sheets, 23 by 15 centimetres, or about 9 by 6 inches, which are perforated with a cutting-punch near the left margin, at distances of 13.5 centimetres from the right margin, and 2.5, 7.5, 16, and 21 centimetres from the lower edge.[1] Any number of these can be bound together by shaking them into place, and passing a twine or thread through the perforations, which all correspond. Slips, of whatever size or shape, not exceeding 23 by 13.5 centimetres in size, can be lightly attached to the right-hand pages by mucilage on two or more corners of the slip. These can be extended, rewritten, or removed, without removing the sheets to which they are attached. The whole of the left-hand page serves for catch-words, classificatory headings and sub-headings, or whatever matter of similar character may be desired, referring to the reverse of the page. New leaves can be inserted, or old ones removed; in a short time, while at all times the notes have the advantage of being in book form, and free from the dangers of accidental displacement, as, for instance, by a gust of wind, or by dropping the package. For rapidity and ease of reference, I know of no better system. The removal of slips from envelopes, and replacing them, take a great deal of time; and the keeping of slips in card-catalogue form prevents a rapid survey of the material in hand. If it is desired to spread the whole material out on one surface, the strings can be withdrawn from the leaves.

The same manila sheets can be used for mounting newspaper scraps for permanent preservation; and pamphlets, circulars, etc., can be perforated with corresponding holes, so that all may be tied together in any sequence desired, and temporary covers, similarly perforated, may be placed on each brochure.

B. PICKMAN MANN.

Washington, D.C., Jan. 19, 1885.

[1] For an article by me on standard holes for temporary binding, see *Library journal*, January, 1883, vol. viii. pp. 6, 7.

THE DECADENCE OF SCIENCE ABOUT BOSTON.

A BOSTONIAN, proud of the scientific fame of his native place, and yet only too familiar with empty benches at the ordinary scientific assemblages, and to whom the election of new members, 'postponed for want of a quorum,' is a standard event, when he visits Baltimore and Washington, begins to ask whether the sceptre has not departed from Israel. He is thereafter a little shy about inviting a brother physicist from Baltimore to attend a meeting of the academy, or taking a naturalist from Washington into a session of the natural history society. To a friend about to visit the national capital, he unburdens himself with sad forebodings of the decadence of science at home; but 'tell it not in Gath,' he whispers as he parts. Nevertheless, it is an open secret.

The actual state of things is simply this, — that the meetings of scientific societies at Washington and at Baltimore are much more numerous and more specialized than at Boston and Cambridge, and present at nearly every session a more varied and interesting assortment of papers, which receive wider and freer discussion at the hands of much larger audiences. So far as interest and attendance go, the meeting in the southern city is what it formerly was in the northern; and it is a pleasant and yet sad reminiscence of earlier and better days for a scientific man from Massachusetts to visit his confreres at the south. He sees again the freshness and eagerness he was wont to see at home. The audience does not sit around the rear door, hat in hand.

It is not easy to see the exact reason for all this changed aspect of affairs in the north. Assuredly, never was more expected of science than at the present day. All men attend her words. Is it that each specialist has become so engrossed in the little corner of the universe he cultivates that he can scarcely see beyond that corner, and must needs keep to it even when he shows its products? Yet why should one's mental horizon be narrower at Boston

than at Washington, at Cambridge than at Baltimore? The only way we can account for this is in the undoubtedly freer social life at the south, by which men are brought into more frequent collision, with consequent interchange of ideas; and this would lead one to conjecture, that, unless manners change, Boston and Cambridge cannot regain preeminence.

It is all very well to say with a complacent air that science does not depend on the public, and that her great discoveries are made far from the noisy world. It is only in exceedingly rare instances that they have been made by men whose scientific ardor was not born of contact with living teachers. And men who seek wisdom for themselves alone defraud the public; especially in these latter days, when it is this very public that is to furnish their successors in the investigation of nature. The public covets no man's scientific gold or apparel, but has a not altogether unwholesome yearning for a sight of it; and it is a travesty of the scientific spirit to keep it from view. Science may be a mild hermit: she can never be a miser.

But to return to Boston. The decadence noticed within the last ten years cannot be attributed to any change of general manners in the modern 'Athenian,' but must be sought in other local causes, and may be largely apparent. The increasing proportion of scientific men residing outside of Boston itself has much to do, during the colder and stormier season, with the small attendance at meetings which it takes an hour's travel to reach; and yet it is rare to find at any scientific gathering in Boston, even if it be an attractive feast, any less proportion than one-half from Cambridge. The university, too, makes larger and larger demands upon its servants; and the extraneous attractions of Cambridge itself, not to mention those of Boston, absorb more and more the time and strength of those who were wont in former years to add to the interest of the scientific meetings in Boston. Their example is followed by their juniors, and Boston itself fails to make good its own loss.

THE GEOLOGY OF THE SCOTTISH HIGHLANDS.

THE geology of the Highlands of Scotland has a peculiar interest for American students, first, because that region has many resemblances, both stratigraphical and lithological, to parts of eastern North America; and, second, because therein the same great questions which have been raised and settled with regard to New-England rocks, have there also been debated and finally solved, with similar results. There is in north-western Scotland an ancient gneissic series, which the present writer, in 1855, pointed out as the equivalent of our older gneiss, as seen in the Laurentides and the Adirondacks. Resting upon this Laurentian or Hebridean gneiss in Scotland, there is found to the east a group of quartzites and limestones containing a lower paleozoic fauna, in part, at least, Cambrian in age; while apparently overlying these fossiliferous rocks, on their eastern side, is a great series of gneisses and mica schists, rising into hills which form the western Highlands, extending south and east, and covering an area of at least fifteen thousand square miles. This whole region was studied a quarter of a century since by Murchison, aided by Ramsay and Harkness, and later by A. Geikie; and in 1858 and 1860 it was declared by Murchison that the gneisses and mica schists of the Highlands were newer than the fossiliferous strata, and were, in fact, rocks of Silurian age in an altered or metamorphic condition. As I pointed out in 1860, the parallelism between these Scottish rocks and those of New England and eastern Canada is evident. The ancient gneiss of the Adirondacks, the paleozoic strata of the Champlain basin, and the crystalline schists of the New-England Highlands, then regarded by most American geologists as of paleozoic age, are a counterpart of the strata of north-western Scotland, and I am aware that Murchison was sustained by these resemblances in his view of the age of the Scottish Highlands. It was, however, then opposed by Nicol, who maintained that these rocks, though distinct from those of the west coast, were, nevertheless, more ancient than the fossiliferous Cambrian found along their western base. I at that time shared the common belief of the metamorphic school of American geologists, and, extending it to the Scottish rocks. supported the thesis of Murchison and his colleagues against that of Nicol. When, however, I became satisfied of the errors of this school, and asserted the pre-Cambrian age of the various

groups of crystalline schists of the Atlantic belt in North America, I declared, in an address before the American association for the advancement of science, in 1871, my conviction that the crystalline schists of the Scottish Highlands "will be found . . . to belong to a period anterior to the deposition of the Cambrian sediments, and will correspond to the newer gneissic series of our Appalachian region." My studies of these, and of similar crystalline rocks in North America, in the British Islands, and in continental Europe, served in succeeding years to confirm this conclusion as to the gneiss of the Highlands, which was again asserted before the geological society of London in 1881.

Meanwhile the attention of able workers in Great Britain had been turned to this great problem in Scottish geology, beginning with Hicks in 1878, and followed by Callaway and Lapworth, all of whom labored independently of each other, but with concordant results. Their separate conclusions, as announced from time to time, but more fully in 1883, agreed in showing that the views of Murchison and his followers were altogether untenable, and in disaccord with the facts of stratigraphy. According to the results of these observers, published early in 1883, there are seen in the Highland region an older granitoid or Laurentian gneiss, and a younger series, consisting in large part of tender gray gneisses and granulites, with mica schists, which are the characteristic rocks of the Highlands, and have been variously named Upper Pebidian, Grampian, and Caledonian. They are indistinguishable from the younger gneisses of the Alps, and from the Montalban of North America, to which they were already referred in 1871. The unconformable superposition of the younger upon the older gneissic series, and the fact that the Cambrian strata rest unconformably upon both, and are younger than either, are also shown. The existence of great parallel north and south faults, with upthrows on their east sides, bringing up successively higher rocks; the fact that these faults pass into sigmoid flexures, in which both the younger gneiss and the Cambrian rocks were involved; and also that the younger gneiss is made to overlie the latter by dislocations, which were accompanied by a great thrust from the east, throwing both series into a succession of folds overturned to the west, giving the whole region a general eastern dip, — were made apparent, as may be seen in the various papers of Hicks, particularly that in the *Quarterly geological journal* for May, 1883, with appended notes by Bonney, and in the papers in the *Geological magazine* for the same year, by Callaway and by Lapworth, the latter entitled 'The secret of the Highlands,' besides a later one by Callaway in the same magazine for May, 1884, on Progressive metamorphism. An abstract of these results will be found in a chapter on the progress of geology, in the Smithsonian report for 1883.

The publication of these conclusions impelled the geological survey of Great Britain to direct its attention to the region for the purpose of defending, if possible, the previously expressed opinions of the official geologists; and, after investigations carried on in 1883 and 1884, the conclusions of the director, A. Geikie, and of his assistants, Messrs. Peach and Horne, are given in *Nature* for Nov. 13, 1884, and reprinted in the *American journal of science* for January, 1885. Therein, while making no special reference to the results obtained by his immediate predecessors, Geikie abandons entirely the views hitherto held by him in common with Murchison and Ramsay, and confirms those of Hicks, Callaway, and Lapworth. He writes that he has "found the evidence altogether overwhelming against the upward succession which Murchison believed to exist in Eriboll from the base of the Silurian [Cambrian] strata into an upper conformable series of schists and gneisses," and adds, "that there is no longer any evidence of a regular conformable passage from fossiliferous Silurian [Cambrian] quartzites, shales, and limestones, upwards into crystalline schists, which were supposed to be metamorphic Silurian sediments, must be frankly admitted." The same conclusions are also reached by Geikie from the re-examinations of similar sections in Rossshire, previously described by himself in accordance with the views of Murchison. The preliminary report of Messrs. Peach and Horne, with a general section, explains the structure in complete accordance with the statements already made by late observers, as explained above.

Geikie, in the paper just cited, calls attention to the laminated and schistose structure developed by the great pressure and friction along the lines of movement in the displaced gneissic and hornblendic rocks, and also to similar changes produced by the same agency in detrital rocks, such as arkose in this region. Both of these structural alterations are apparently included by Geikie under the head of what he calls a 'regional metamorphism.' This, however, is a misapplication of the term, likely to confuse and mislead the reader, since local

structural changes induced by mechanical movements in ancient crystalline rocks have nothing in common with that mysterious process which has been supposed by the metamorphic school to generate similar crystalline rocks from non-crystalline sediments. As regards the changes wrought by the same agency in detrital masses, it may be repeated that "the resemblances between primitive crystalline rocks and what we know to be detrital rocks, compressed, recemented, and often exhibiting interstitial minerals of secondary origin, are too slight and superficial to deceive the critical student, and disappear under microscopical investigation."

The doctrine of a regional and progressive metamorphism as the origin of the crystalline rocks, which was very widely received a generation since, both in Europe and America, has within the last fourteen years become greatly discredited. In the Alps, where it was first seriously applied, as well as in Great Britain, it is now generally abandoned. Callaway wrote not long since, that "every case of supposed metamorphic Cambrian and Silurian has been invalidated by recent researches;" and Bonney, now president of the Geological society of London, declared, in 1883, that the hitherto accredited "instances of metamorphism in Wales, and especially in Anglesea, in Cornwall, in Leicestershire, and in Worcestershire, have utterly broken down on careful study," as had already been the case in the Alps, and, it may be added, in North America. The official geologists in Great Britain, representing the traditions of the old school, have, however, hitherto held to the Scottish Highlands as their last stronghold, which they are now forced to abandon, — a substantial victory for rational geology.

T. STERRY HUNT.

Montreal, Jan. 10.

THE BASIN OF THE CARIBBEAN.

THE U. S. hydrographic office having sent to the New-Orleans exposition, as part of its exhibit, a model of the Caribbean Sea, it will be interesting at this time to discuss the deep-sea soundings taken by officers of the navy in the coast-survey steamer Blake, and in the fish-commission steamer Albatross, from 1878 to 1884, by means of which this model was constructed.

Particular attention was called to this great basin in the coast-survey reports for 1880 and 1881, and also in a paper read by the writer before the American Geographical Society in the winter of 1882.

It was not possible, however, to give the contour of the bed of this sea until the completion of the work of the Albatross last winter. The data then obtained permitted the construction of the accompanying chart, which is a faithful representation of the model before mentioned, and by means of which it will be easy to draw attention to some of its most important features.

During the cruise of the Challenger, it was demonstrated that in a submarine lake the temperature is constant to the greatest depth, and the same as that of the ocean at the depth of the rim of the lake at its lowest or deepest point.

The investigations of the temperature of the Gulf of Mexico by Commander Sigsbee, from 1874 to 1878, had shown that below a depth of 800 fathoms the temperature is constant at $39\frac{1}{2}°$, which is the normal temperature of the ocean at that depth in the region of the Equatorial Current. It was evident, therefore, that the Caribbean Sea, from which the Gulf of Mexico receives its waters, must be enclosed by a rim which at its deepest part was 800 fathoms below the surface.

The purpose of the investigations of the Blake, during the time that I had the honor to command, was to verify the deduction thus made, and to determine the position and height of this rim, which limits the low temperature of the waters of the Gulf of Mexico.

All the passages between the islands from Trinidad to Cuba were carefully sounded, and the existence and position of the rim definitely established. At the same time temperatures were taken both outside and inside the basin, and at the points of minimum depth. With one exception, however, the only place where the rim was sufficiently low to admit water of the required temperature ($39\frac{1}{2}°$) was in the windward passage. In all other places the depths on the rim were much less than 800 fathoms.

The exception noted was a narrow gully of 1,100 fathoms, with a bottom temperature of 38°, leading into a basin of 2,400 fathoms between Santa Cruz and St. Thomas; this great depth also having a bottom temperature of 38°. As the temperature at 1,500 fathoms just south of Mona Passage was $39\frac{1}{2}°$, there could be no doubt of the existence of a rim from Santa Cruz to Puerto Rico. The Albatross, therefore, was directed to examine this locality, and, as was expected, found the ridge with 900 fathoms on it at the greatest depth, and a least

temperature of $39\frac{1}{2}°$. This established the continuity of the rim, and consequently the truth of the deduction made from the examination of the temperatures of the waters of the Gulf of Mexico.

That part of the Caribbean Sea west of the Island of Jamaica, and the Pedro and Rosalind banks, were thoroughly sounded in the winter of 1880–81; and the Cayman Islands and the Misteriosa Bank were found to be part of a submarine extension of the range running along the south-east side of Cuba.

Immediately south of these partially submerged peaks was an immense deep valley, extending from between Cuba and Jamaica as

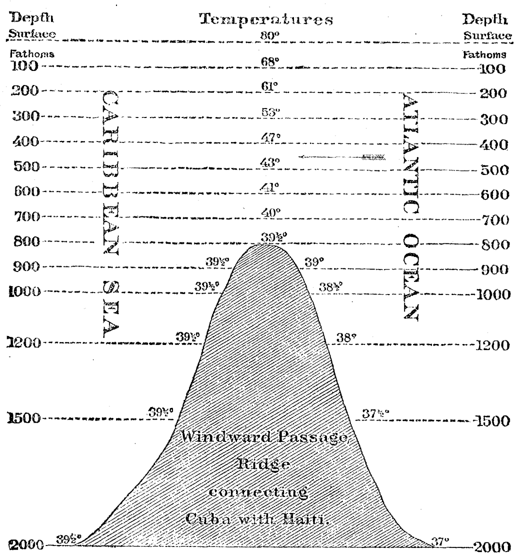


DIAGRAM OF RIDGE CONNECTING CUBA WITH HAYTI.

far as the Gulf of Honduras. This valley is narrow at its eastern end, but widens between the western end of Jamaica and Cape Cruz, where the soundings were 3,000 fathoms within fifteen miles of Cuba, and 2,800 fathoms within twenty-five miles of Jamaica. This valley is 700 miles long, with an average breadth of 80 miles. The greatest depth was 3,428 fathoms, just south of the Island of Grand Cayman. Between Misteriosa Bank and Chinchorro Bank, the soundings were regular at 2,500 fathoms. North of Misteriosa and Grand Cayman, to the Isle of Pines and Cape San Antonio, the soundings were generally 2,500 fathoms. So much for the western Caribbean.

The lines of deep-sea soundings taken by Lieut.-Commander Tanner in the Albatross last winter, were first from the east end of Puerto Rico to Bird Island, thence nearly south to Trinidad, and then north-west towards the Mona Passage. These lines, in connection with that which I ran with the Blake from the island of Dominica to Bird Island, and back to Monserrat, clearly developed a submarine elevation reaching north and south nearly parallel to the main chain of islands from Granada to St. Christopher; the depth on this ridge being considerably less than 1,000 fathoms, with 1,500 and 2,000 fathoms on each side.

From the Mona Passage a line was run to Los Roques, thence to the mainland, and then to Curaçoa. The soundings south of this chain of islands gave a greatest depth of 1,030 fathoms. A line was run from Curaçoa to Beata Island, at the extreme south point of Santo Domingo. A line was also run from Morant Cays, off the east end of Jamaica, to the mouth of the Magdalena River, and then across the Rosalind Banks.

These several lines show an immense basin of nearly the same depth, extending from Santo Domingo and Puerto Rico to the shores of the Spanish Main, and over an area of more than 200,000 square miles, without any apparent inequality of surface. A line drawn from the west end of Santo Domingo to Cartagena shows a depth of 2,200 fathoms. The floor of the basin then rises gradually to the banks connecting the island of Jamaica with the Mosquito Bank.

The basin is a few hundred fathoms deeper in its eastern part, but rises abruptly to the submarine elevation previously mentioned. A very remarkable depression will be observed in the Atlantic, north of Puerto Rico. Lieut.-Commander Brownson here obtained a depth of 4,561 fathoms. Additional soundings will be taken, when a vessel is available, for more details over the ridge of which Bird Island forms a part; but the general basin is probably correctly portrayed.

The soundings connecting the islands and various banks, and to determine the depths of the western Caribbean, have already been made in detail. This work, so eminently within the province of the navy, and performed with so much success by naval officers, should be continued, and I hope to obtain many new soundings this summer.

The model also shows the elevations of adjacent shores and islands. The horizontal scale is 33 miles to the inch; vertical, 6,000 feet or 1,000 fathoms to the inch. The latter, though so much distorted, was necessary to give the smaller elevations and depressions.

Even with this scale, the highest mountain

on the Isthmus of Panama was only one half-inch, and the elevation of the railroad less than one-twentieth of an inch. Again: the mountain of Santa Marta, near Cartagena, was 17,000 feet, or nearly three inches in height; but the whole gave relative heights which could have been shown in no other way.

J. R. BARTLETT.

U. S. hydrographic office, Jan. 15.

THE BALLOON IN METEOROLOGY.

ON the afternoon of Jan. 19 the first balloon ascent ever made in this country solely in the interest of meteorology took place at Philadelphia. As the beginning of a series to be carried out strictly for scientific purposes, it was an event of no small importance. Gen. Hazen, chief signal-officer, U.S.A., recognizing the importance and value of a more complete knowledge of the upper atmosphere, entered into a contract some time ago with the well-known aeronaut, Mr. S. A. King, for a number of 'trips to the clouds,' an ascent to be made at any time on eight hours' notice.

Although the first balloon excursion for strictly scientific purposes made in America, this was by no means the first on record. Naturally, very soon after the invention of the balloon, attempts were made to utilize it in meteorological investigations. Doubtless, the first ascents having this end in view were made by Mr. Robinson, from St. Petersburg, at the command of the emperor of Russia, in 1803 and 1804; but it does not appear that any important results came from them. On Aug. 31, 1804, Gay-Lussac and Biot made an ascent, reaching a height of thirteen thousand feet; and meteorological observations were commenced after an elevation of seven thousand feet had been passed. On Sept. 15 of the same year, Gay-Lussac reached a height of twenty-three thousand feet, making a series of most important observations, and bringing air down from that height, which, on being analyzed, was found to have the same constitution as that at the surface.

Not much seems to have been done from that time until 1843, when the British association for the advancement of science appointed a committee and voted a sum of money for the purpose of experimenting with captive balloons. Although the work was continued under several committees, it was not very successful, owing, doubtless, to a lack of skill in the management of captive balloons. In 1850 Messrs. Bixio and Barral made ascents in France for the purpose of meteorological study, in which it was planned to ascend to heights as great as forty thousand feet. They did not succeed, however, in reaching greater elevations than had been attained before, but obtained results verifying in the main those of Gay-Lussac. On one of these excursions an elevation of twenty-three thousand feet was reached; and, in addition to the meteorological work, interesting observations were made on polarization and other optical phenomena.

A series of very important ascents was made by Mr. Welsh of the Kew observatory in August, October, and November of 1852, in which heights varying from twelve thousand feet to twenty-three thousand feet were reached.

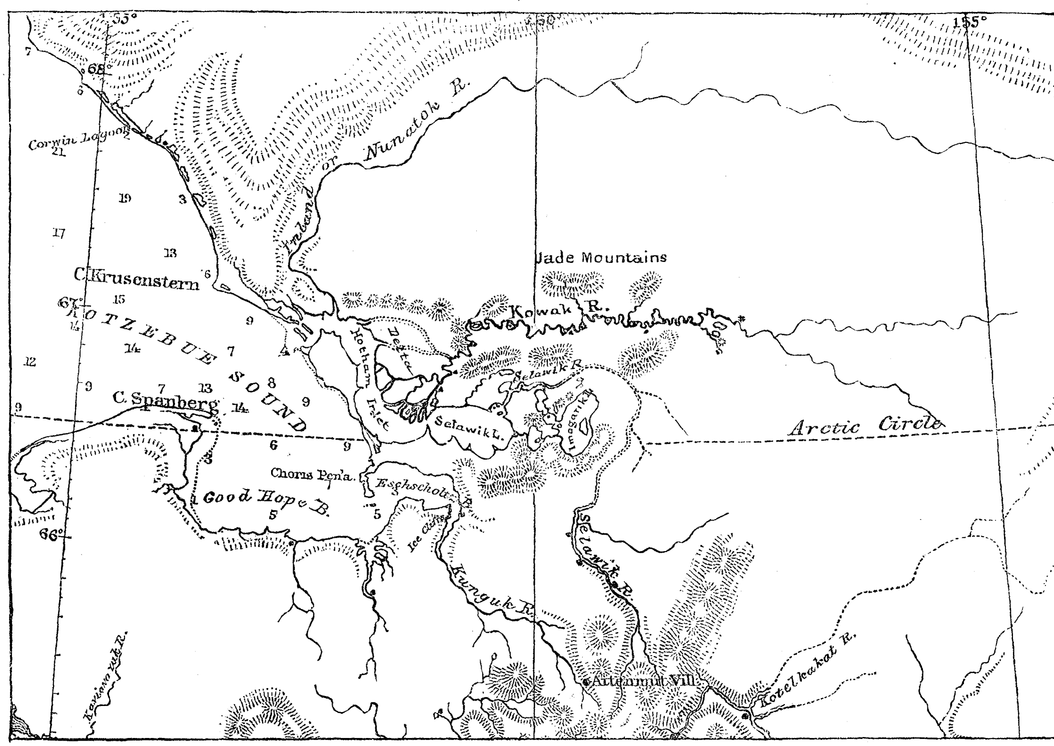
A few years later the interest of the British association in the subject was renewed, and culminated in the celebrated series of ascents made by Mr. Glaisher, the first being on July 17, 1862. In these ascents the most complete arrangements were made for the study of the physics of the higher atmosphere, and they were remarkably successful.

Since that time, scientific ballooning has been carried on with great success in France by Camille Flammarion, W. De Fonville, and Gaston Tissandier. A complete and extremely interesting history of their work (up to the date of its issue), together with that of Glaisher, is to be found in a volume entitled 'Travels in the air,' by James Glaisher.

The U. S. signal-service has had this subject under consideration for several years. Professor Abbe began in 1871 to collect meteorological records made in balloons. In 1872 the records of fifty ascents had been tabulated, studied, and valuable results obtained. In 1876 one thousand small balloons were sent with the Polaris expedition, to be used in determining the height of the clouds; but, owing to an unfortunate accident, they could not be utilized. At various times the chief signal-officer has sent observers on balloon excursions which were made for purposes other than scientific.

The considerable certainty with which the movement of a storm can now be predicted renders it possible and desirable to make systematic use of the balloon in the study of unusual atmospheric conditions, and the series of ascents just begun is planned with that end in view. Among other things, it is desired to determine the difference in the temperature gradient in well-defined 'high' and well-defined 'low' pressures. For this purpose it is necessary to foretell the arrival of a particular atmospheric condition at Philadelphia, from

which place the ascents will be made. This can readily be done so as to give the aeronaut eight hours' notice for the preparation of his balloon, and the observers who accompany him sufficient time to reach Philadelphia from Washington. The first ascent was expected to be rather experimental and suggestive in its character. It was the intention to start at seven A.M., on the 19th; and a telegram to that effect was sent to Mr. King, who responded that he would be ready. But, owing to the extreme hour of starting, the observations made were not so numerous as could be desired, although seven complete sets were obtained before darkness rendered further reading impossible. A safe and quiet landing was effected at about half-past seven P.M., near the village of Manahawken, on the New-Jersey coast. The greatest height reached was somewhat over one mile. This trial-trip has suggested some modifications in the plans, which will render future ascents more successful. The danger incident to a



THE NEW SURVEYS OF THE KOWAK RIVER, ALASKA.

cold, it was found that the balloon could not be handled for filling without danger of cracking; and waiting for the sun to warm it up caused so much delay, that the start was not made until quarter-past four P.M. The balloon was the Eagle Eyrie, holding twenty-five thousand cubic feet when filled, and having a lifting-power of about a thousand pounds. The occupants of the car were Mr. King and Private Hammond, a skilful observer detailed from the office of the chief signal-officer for the purpose. Mr. Hammond carried with him a complete outfit for making barometric, thermometric, and hygrometric observations. Owing to the late

balloon ascent is greatly over-estimated by many. In the company of an experienced and skilful aeronaut the risk to life and limb is hardly greater than on a railway-train or a steamboat. Mr. Green, the famous English aeronaut, made fourteen hundred ascents, and lived to be eighty-six years old. The excursion of the 19th was the two hundred and fifty-eighth made by Mr. King. Volunteers for this service are by no means wanting among those connected with the signal-service; and Professor Abbé is so desirous of knowing what is going on 'inside of a storm,' that he means to make an ascent himself, in order to find out.

Altogether, this systematic use of the balloon for the study of special meteorological conditions must be regarded as a new departure; and the signal-service is to be congratulated on its successful initiation.

THE KOWAK RIVER.

THE map opposite shows the explorations made by the U. S. revenue marine on the Kowak or Kūak River during the season of 1884. The asterisk indicates the farthest explored point on the river. The native settlements are shown by small black triangles. The course of the lower part of the Selawik River and part of the Kowak delta, indicated in dotted lines, have not been explored. It will be observed that the new explorations almost exactly join the course of the river as laid down on the coast-survey map of 1884 by Dall, from Woolfe and Jacobsen's sketch-map. The spelling of the names on the above map has not been modified to agree with the Innuit pronunciation as obtained by Lieut. Cantwell, since the different tribes of the region do not pronounce these names uniformly, and the names 'Kowak' and 'Selawik' have been adopted on all charts for many years. According to Lieut. Cantwell, the people of the river call it Kū-ak (or 'big river'). Other names are Shēlāwik (Selawik, or 'fish') lake and river, Imogarik′-choit (lake or 'little sea'). The stream connecting this with Selawik River is Ig′-yăk ('throat') River: that flowing to Selawik Lake is Ki-ăk′-tŭk ('fox') River. Others have been referred to in our report of this exploration. It is probable that the upper part of the Selawik, taken from the Western union explorations of 1866–67, is too far to the westward, and that the course of the river is less irregular than above indicated; but there are not sufficient data to make this certain, or to alter the chart at present.

A GLANCE AT THE HISTORY OF OUR KNOWLEDGE OF FOSSIL PLANTS.[1]

THE ancients, though acquainted with fossil shells and corals, were wholly ignorant of fossil plants; and the first mention of any vegetable substance in a state of petrifaction was made by Albertus Magnus about the middle of the thirteenth century. Agricola, Gesner, and others treated of petrified wood in the sixteenth century; and, during the seventeenth, Major in Germany, and notably Lhwyd in England, called attention to the existence of vegetable impressions in the rocks. By the beginning of the eighteenth century considerable collections of such material existed in the European museums, and this had become the subject of animated discussion. Dendrite had long been known, and was then generally supposed to represent vegetable matter; but in the year 1700 Scheuchzer overthrew that doctrine, and established its purely mineral character.

Prior to this date the prevailing notions of the times ascribed all fossils to some mysterious cause, and denied their reality as the remains of things that had once possessed life. As to their true nature, there was, however, no harmony of opinion. Some looked upon them as divinely created archetypes of living things, others as divine enigmas placed before man to test his faith, others still as merely the varied forms of the subterranean world corresponding to those of the earth's surface, while many regarded such objects as purely accidental, or as mere freaks of nature.

Against these predominant mystic views there had, however, long existed the theory that these forms, so strikingly similar to real things, might be the petrified remains of the life that perished by the Noachian deluge, and which had been stranded on the mountains and highlands of Europe and Asia. This view was countenanced by Martin Luther, and strongly defended by Alexander ab Alexandro in the sixteenth century; while towards the close of the seventeenth it secured many earnest advocates, including Woodward of England, and Scheuchzer of Switzerland. The latter undertook to defend his theory from the evidence furnished by plant-remains; and from this zeal resulted his greatest work, one of the most remarkable of the time, — his 'Herbarium diluvianum.' This appeared in 1709, and in it are enumerated and figured many fossil plants. These impressions were declared to be those of existing and often familiar species; and we find among them the myrrh of Scripture, Galium, Hippuris, and other well-known forms. So confident was Scheuchzer that these were living plants, that in 1718 he ventured to classify all known impressions according to Tournefort's system, as drawn up in his 'Elémens de botanique' in 1694. The new edition of the 'Herbarium diluvianum,' which appeared in 1723, contained this systematic table, in which four hundred and forty-five species are enumerated.

This bold stroke aroused an intense interest in the subject, and immediately led to a closer comparison of the fossil with the living flora. In this work, Leibnitz in 1706, and Antoine de Jussieu in 1718, had already led the way by examining certain well-defined impressions, and expressing strong doubts of their identity with any European species. Further investigations were made; and these disagreements soon gave rise to the belief that they were tropical forms which by some convulsion or vicissitude had been brought to Europe, and buried under its soil. This view prevailed until the close of the eighteenth century.

Thus far the idea of ancient or extinct life had

[1] Read before the American association for the advancement of science, Sept. 8, 1884, by LESTER F. WARD.

scarcely been conceived; but continued failure to correlate fossil with living forms, even after thorough examination of many tropical floras, began to give importance to this question, and in the first year of the present century Baron von Schlotheim commenced to urge for plants, what Blumenbach had for some years insisted upon for animals, that the fossil forms were extinct, and belonged to another age of the world, characterized by a different kind of life. Hard as this doctrine then was for the beliefs of the times, its manifest soundness caused it steadily to gain ground, and soon opened the way for the serious study of paleontology on a true scientific basis.

The reaction against attempting to correlate fossil with living plants went too far, and the former nomenclature was completely abandoned. Judging all by the paleozoic forms, which had been the chief objects of study, all efforts to apply generic names even to those of the most recent formations were suspended, and resort was had to the terminologies of the mineralogists, particularly those of Waller, Walch, and Schröter. All vegetable remains were called phytolithes. Impressions on the rocks were distinguished as phytotypolithes. Fossil leaves were named bibliolithes, and fossil fruits carpolithes. Not until 1818 did any one venture to establish species under any of these heads. The first attempt of this nature was made in that year by the Rev. Henry Steinhauer, whose now celebrated memoir, 'On fossil reliquia of unknown vegetables in the coal strata,' describes and figures ten species of Phytolithus, assigning to each an appropriate specific name. This may be regarded as the true birth of systematic paleobotany, — an example of the humility of true science as contrasted with the arrogant assumptions of Scheuchzer a century before.

It is remarkable that this initial paper by Steinhauer was published in an American serial, the Proceedings of the American philosophical society, at Philadelphia, and was contributed by an American citizen, and member of that society. But that it was founded on any extensive study of the coal-plants of this country, as some have stated, there is no internal evidence. No American localities are mentioned; and the paper seems to deal throughout with British fossils and British coal-mines, with which the author was perfectly familiar.

Schlotheim, who in his 'Flora der vorwelt,' 1804, had not dared to go thus far, took a step in advance, two years later, in his 'Petrefactenkunde.' He greatly enriched the terminology of the science, and described with true binomial designations seventy-eight species belonging to seven genera of fossil plants.

Count Sternberg's 'Flora der vorwelt' commenced to appear in parts at about this time, in which many new genera were created on thoroughly studied grounds; and in 1822 Adolphe Brongniart's elaborate paper on the classification of fossil plants was published in the memoirs of the Paris museum of natural history. But these contributions, though highly systematic, and by far the most important that had been made to the science, did not descend to the question of species, nor indicate the number of distinct forms. The next work, therefore, in which light is thrown upon this problem, was Brongniart's 'Prodrome,' which appeared in 1828. By this time the science of paleontology had been fairly established, and geognostic considerations had come to receive something like their due weight. The ancient floras were distinguished from the later ones, and the approaching analogy of the latter to that of our own time was clearly perceived by Brongniart, who thus early prophetically declared for the successive development of higher types, though this view was strenuously opposed by the English school a decade later.

In this work, and the large treatise published the same year ('Histoire des végétaux fossiles'), to which it forms an introduction, an immense advance was effected in the systematic treatment of fossil plants. Not only was a large number of species recognized, belonging to the extinct genera heretofore established, and many new genera created, but the identity of many of the fossil with living genera was boldly asserted, at least for the more recent formations; and a long step was taken in the direction of correlating the extinct and living floras, and of demonstrating the fact of an uninterrupted series connecting the past with the present plant-life of the globe.

At that date Brongniart enumerates five hundred and one species of fossil plants, nearly half of which belonged to the first, or oldest, of his four periods, corresponding to the paleozoic of modern geologists, and of course chiefly from the coal-measures.

It is interesting to note here how much faster the science of fossil plants has advanced in this numerical respect than that of botany proper; for, while more than a hundred living species were then known to Brongniart for every fossil species, only eighteen living plants are now known to one fossil plant. And yet how rapid has been the growth of our knowledge in both sciences may be realized by contemplating the fact that nearly five times as many living, and sixteen times as many fossil, plants are recognized now as then.

A census of fossil plants was again taken in 1845, by Unger, in his 'Synopsis plantarum fossilium,' in which he enumerates 1,648 species; and in the same year, by Göppert, quite independently of the former work, in a paper published in Leonhard and Bronn's 'Neues jahrbuch für mineralogie,' in which 1,778 species are claimed. Sixty-eight thousand living species were then known to Göppert, or about thirty-eight living to one fossil species.

In 1849 Göppert again reviewed the fossil flora, and published an exhaustive enumeration in Bronn's 'Index palaeontologicus.' He now finds 2,055 fossil species, to be compared with the 69,403 living species named in the same work, or less than thirty-five living to one fossil species.

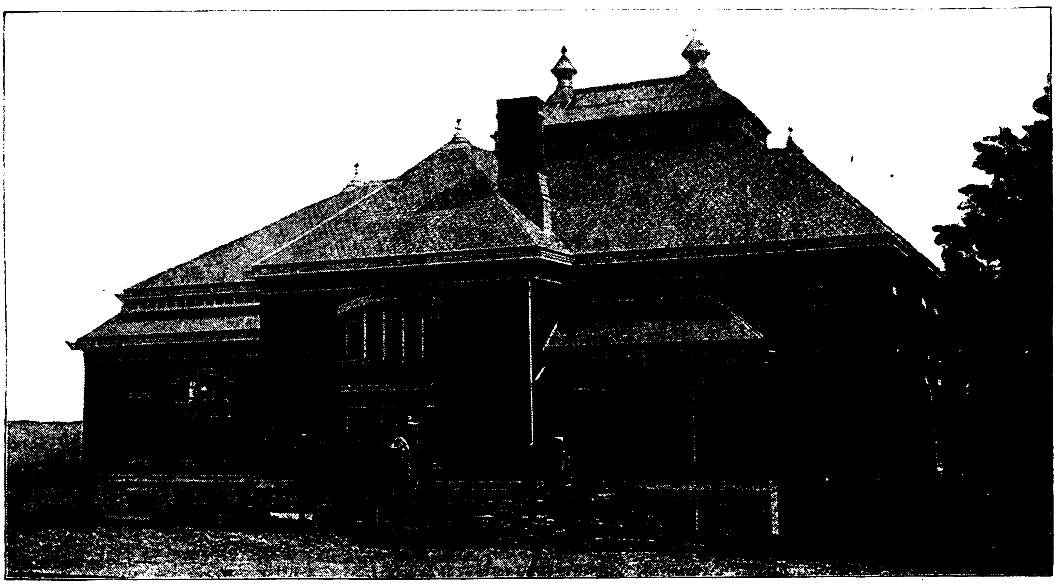
The third quarter of the present century was one of intense activity for systematic vegetable paleontology. The combined labors of Heer, Saporta, Ettingshausen, and Lesquereux, with a large corps of

co-laborers working upon abundant material from all parts of Europe, from the arctic regions, and from the United States, multiplied several times within a few years the number of fossil plants known to science; so that by the time of the completion of Schimper's 'Traité de paléontologie végétale,' in 1874, he found that he had been able to describe in that work about six thousand good species, after a liberal exclusion of uncertain forms. But a thorough inspection of this important work shows that even then he came far short of gathering in all the data extant at that date, while it is since then that most of the solid work in this line has been done in America and in the polar districts.

A catalogue of all the fossil plants that have been described, down to the present year, is in preparation at the National museum; and, though still far from complete, the work has sufficiently progressed to warrant an approximate estimate of the present number of species, which cannot fall far short of nine thousand, and may considerably exceed that figure.

PHYSICAL TRAINING AT AMHERST.

THE recent inauguration of the new health-building at Amherst college is a noteworthy feature in the development of this department of collegiate institutions in general. Amherst college was, it will be remembered, the first institution of the kind in America to awaken to the practical necessity of a competent physical culture proceeding simultaneously with the intellectual development of its students; and effective measures were taken to secure these ends nearly thirty years ago, soon after the accession of the late Dr. Stearns to the presidency of the college, when, in the year 1859, the board of trustees created the department of physical education and hygiene. Prescribed physical training four times weekly was constituted a part of the regular college course, and has been maintained under the immediate personal superintendence of a regularly educated physician, who exercises, in addition, a general oversight of the health of the college. And it is worthy of note here, that, while the experience of similar institutions elsewhere has often been very different, no epidemic has visited this college for the past twenty-five years, nor has any serious or permanent injury ever happened from the gymnastic exercises, either required or voluntary. From the outset the department which had to do with the physical education of the student has been on equal footing with the other departments of collegiate instruction, and the facts of the relative attendance upon the required exercises in light gymnastics show that this position of the department is fully and cheerfully recognized by the students.



THE NEW AMHERST GYMNASIUM.

While in the conduct of the affairs of the new health-building, or Pratt gymnasium, — the gift of Mr. Charles M. Pratt of Brooklyn, — no radical change is contemplated, there is, with a greatly larger structure, more completely specialized apparatus, and all the conveniences for promoting bodily health as well as fostering physical development, a vast field for amplification of the work of the department which it is now in the strongest position to occupy. The interior arrangements of this structure present much that is new in college gymnasiums; and nothing has been spared to provide the most suitable forms of

every thing useful. In addition to the attendance upon the prescribed gymnastic exercises, it is found that a large proportion of the students, of their own accord, make use of the facilities here afforded for the acquirement of a complete physical development, and the maintenance of good health.

Not among the least of the far-reaching results of the work of this department is the uniform series of vital statistics obtained from all students of the institution, and which consist of a permanent record of certain bodily measurements and tests of the vital organs made three times during the course of the student at college. Since the inception of the department, nearly three thousand different men have been measured on a systematic plan, and the results have already formed the basis of invaluable contributions to anthropometry. The accompanying illustration is reproduced from a photographic view of the north front of the gymnasium.

SCIENCE AND SURGERY: A TRIUMPHANT RESULT OF EXPERIMENTAL RESEARCH.[1]

FROM the earliest ages, the functions of the brain have been a fascinating study to cultivated minds, and the greatest intellects of all ages have occupied themselves in attempting to solve its difficult and complicated problems. With the ancients this was a favorite pursuit, and engrossed the thoughts and talents of their most illustrious philosophers. Owing to the absence of exact methods of scientific observation and experiment, the conclusions on this subject were for many centuries of a purely speculative character, and the errors and fallacies thus deduced have been handed down and accepted till comparatively recent times.

Modern investigations have, however, thrown a flood of light on the question; and, although much still remains in the dark, the former obscurity has of late years been brightly illumined by the lamp of science. The accumulated clinical experience of ages had left knowledge on the cerebral functions in a state of confusion and uncertainty; and, owing to the obvious difficulties and complications associated with disease, the results, however significant, were at best imperfect. That the brain should be subjected to direct physiological experiment, was, until modern times, never attempted. During the last generation only, has the practicability of this been demonstrated; and numerous observers have, by direct operations on the brain-substance of animals, arrived at new conclusions as to its functions, and greatly revolutionized our ancient conceptions on the subject. Evidence has also been given against the *noli me tangere* theory, and abundant proof has been adduced of the fact that the brain may be handled, irritated, or partially destroyed, without necessary danger to life.

One of the latest developments of this method of investigation has been the discovery of those centres in the cortex which preside over voluntary motion, which have been, more especially by Professor Ferrier, differentiated and localized with great precision. This important knowledge has been arrived at by an extended series of experiments conducted on living animals, in which, by observing the several effects of stimulating or destroying limited areas of their brains, the different functions of these special localities have been determined. A topography of the cerebrum has thus been constructed, in which the various faculties have been mapped out; but these, unlike the illogical visions of the phrenologists, have stood the test of sceptical criticism and rigid experimental inquiry.

Researches of a purely scientific nature, carried out only with the object of elucidating truth and advancing knowledge, without immediate prospects of material gain, have in this instance led to most important and useful practical advantage. Armed with the knowledge acquired on animals in the laboratory, the physician has been enabled to utilize at the bedside the conclusions thus arrived at for the service of human beings. Clinical experience, combined with morbid anatomy, had already enabled the medical man to suspect the presence of disease in the brain; but as to its precise locality he was formerly in doubt. Now, however, guided by the recent revelations of physiology, he is enabled to predict the position in a large number of cases with great certainty and precision.

Evidence of this is afforded by the proceeding adopted in a case of disease, notice of which has lately appeared in the medical papers. It appears that a man presented a series of symptoms which enabled Dr. Hughes Bennett to diagnose a tumor of the brain, that it involved its cortical substance, that it was probably of limited size, and that it was situated at a certain definite spot. The skull was trephined over the suspected region: there a tumor was found and removed. On recovering from the immediate effects of the operation, the patient was, and continued for three weeks, in a satisfactory condition. He was perfectly intelligent; his functions, except for certain defects of motion caused by the disease, were normally performed; and there was an absence of all the distressing symptoms from which he had formerly suffered, and from which he must necessarily soon have succumbed. Unfortunately, at the end of this time a complication incident to all serious surgical operations supervened, from which the patient ultimately died. The unhappy termination of this particular case does not in any way detract from the importance of the principles which it involves. It still remains a signal triumph of diagnostic accuracy, — a precision mainly attained by exact experimental research. It is, moreover, further proof, that, by utilizing this improved knowledge, the surgeon may not only remove disease from the brain, but may do so without necessary shock or risk to the nervous system; and that the procedure, under modern antiseptic precautions, need not be attended with greater danger than may follow any other severe surgical injury.

[1] Reprinted from *Nature* of Jan. 8.

This interesting and instructive case will doubtless inaugurate a new era in medical practice; for, although this particular individual has succumbed to measures adopted to avert his otherwise certain death, the experience thereby gained is sufficient to encourage further efforts in a similar direction, which may prove beneficial to others. In the Marshall Hall oration of last year, Professor Ferrier remarked, "There are already signs that we are within measurable distance of the successful treatment, by surgery, of some of the most distressing and otherwise hopeless forms of intercranial disease, which will vie with the splendid achievements of abdominal surgery." He further added, reflecting on the success which had attended brain operations on animals, "I can but believe that similar results are capable of being achieved on man himself." That distinguished physiologist can but feel gratified that his prophetic words have been partially realized.

DISCOVERY OF SILURIAN INSECTS.

SOME weeks since, we noticed the discovery by Lindström of a Silurian scorpion, Palaeophoneus nuncius, — the earliest-known air-breathing animal. To-day we reproduce in natural size a photograph of it received from Dr. Lindström. How quickly one discovery leads to another, is evinced by the curious fact that we now learn of the discovery by Dr. Hunter of another scorpion of the same genus in the Ludlow beds of Scotland, which are also referred to the upper Silurian. This second specimen, fortunately, is preserved so as to show the stigmata and 'comb' of the ventral surface, and will therefore offer more evidence as to its exact zoölogical position. It is in the hands of Mr. Peach of the geological survey, who described the carboniferous scorpions of Scotland with such care. Even this curious discovery is eclipsed by the announcement, at the last meeting of the French academy in 1884, of the finding of an insect's wing in the middle Silurian of Calvados, which Mr. Charles Brongniart, who announces the discovery, refers to a cockroach. It presents certain peculiarities, and among others an unusually long and straight anal vein. It is named Palaeoblattina Douvillei, after its discoverer. The oldest-known winged insects, up to this time, had been the Devonian insects of New Brunswick.

PALAEOPHONEUS NUNCIUS.

METEOROLOGICAL NOTES.

THE Colorado meteorological association, recently formed, proposes to establish stations for observation at twenty or more points in Colorado, and has applied to the legislature for assistance.

In co-operation with the chief signal-officer, U.S. army, arrangements have been completed with the Old colony railroad, whereby 'cold-wave' flags — white, with a black square in the centre — will be displayed at eleven of the most important stations on the road, on receipt of telegraphic orders from Washington. The stations are Boston, Quincy, South Braintree, Brockton, Middleborough, Taunton, Somerset, Fall River, Newport, New Bedford, and Plymouth. An extension of this arrangement is in contemplation, so as to bring the daily weather forecasts issued by the signal-office into even more general notice than they gain by publication in the daily papers. Postmasters or town authorities in New England, desirous of undertaking the display of daily weather signals, are requested to address Mr. W. M. Davis, Cambridge, Mass.

Investigations upon the subject of ozone and the relation of its presence or absence to epidemic diseases are now carried on in various sections of the country. If sufficient encouragement is given, it is probable that observations will be undertaken by the New-England meteorological society, under the supervision of Dr. E. U. Jones of Taunton, Mass. Physicians and others who would be willing to engage in these observations are requested to address Dr. Jones. The cost will be about three dollars annually for each observer.

On the morning of Dec. 27, when the wind was everywhere light, the temperature at the summit of Mount Washington was +16°, while at stations at lower levels, north of the Massachusetts boundary, the temperatures ranged from —10° to —24°. A more striking instance of the disturbance of the usual law of decrease of temperature with increase of altitude is rarely noted.

In his 'Meteorological summary' for the year 1884, Prof. F. H. Snow states that the most notable features of the year 1884, in Kansas, were the low mean temperatures of the spring, summer, and win-

ter months; the high mean temperature of the autumn months; the very large rainfall, which came within half an inch of the extraordinary precipitation of the year 1876; the unusual percentage of cloudiness; the low velocity of the wind; the decided preponderance of south winds over north winds; and the increased percentage of atmospheric humidity.

The master of the steamship British King, from Swansea, reports, Jan. 15, in latitude 41° north, longitude 67° 10′ west, encountering an electric storm which lasted about four hours. The weather had been overcast with heavy rain from noon until six P.M., when the wind shifted from south-west to west, followed by loud claps of thunder and vivid flashes of lightning. At the same time large balls of 'St. Elmo's fire' were seen on all the yard-arms and mast-heads. All of the stays and back-stays were covered with sparks of fire of a bluish tint.

Professor Kiessling of Hamburg has issued a circular in the name of the Hamburg-Altona branch of the German meteorological society, asking practised observers, accustomed to noting the appearance of the sky, for reports on the colors still visible in the neighborhood of the sun in clear weather, as well as for records of the dates on which these peculiar displays first became visible. He regards them as sequels to the extraordinary twilights of 1883, and considers all these optical effects as results of the Krakatoa eruption. The phenomena on which observations are especially desired are the vaguely defined, smoky, reddish ring enclosing a brilliant whitish disk around the sun; and the pale red tint that has been seen between clouds at a greater distance from the sun, while the solar disk itself was hidden. Observations from distant, out-of-the-way stations are particularly valuable; and the records of mountain observatories are of greater interest than those of lower levels, as the solar diffraction ring is much more distinct when seen in the relatively clean upper air than when viewed through the dust-laden strata of the lower atmosphere. Professor Kiessling has published valuable papers on the optical theory of the brown-red ring in the *Naturforscher* and in *Das wetter*.

In his report on the New-Hampshire state triangulation in 1884, Prof. E. T. Quimby says, "It may be proper to mention that while the 'red sunsets' have not been so marked as they were a year ago, the Krakatoa dust has been constantly and plainly visible from sunrise to sunset every day when the sky has been free from clouds. There has been no day when the sky has had its normal blue."

THE CHEMISTRY AND PHYSICS OF THE SEA.

FORCHHAMMER showed in 1864, by his analysis of several hundred samples of sea-water, that, though the water of the ocean may vary greatly in degree of dilution, the composition of the saline matter in solution is, for surface-waters, and so far as concerns the chlorides and sulphates of sodium, magnesium, and calcium, — the principal components, — constant within the limits of error of his work. Besides these more important constituents, other substances to the number of twenty-four elements are known to occur, but in their entire sum amount to but a small fraction of one per cent of the total saline matter.

In part i. of the volume before us, Professor William Dittmar gives his researches into the composition of ocean-waters collected by the Challenger. Seventy-seven samples, representing different stations upon the ocean, and various depths beneath the surface, yielded figures, which, agreeing fairly well with those of Forchhammer, and better still among themselves, seem to warrant the conclusion that the composition of the salts in sea-water is independent of the latitude and longitude of the station from which the water is taken, and of depth also, so far as concerns the chlorine, sulphuric acid, magnesia, potash, soda, and bromine. The proportion of lime, however, increases with the depth of the water. The following table contains Professor Dittmar's figures for the mean composition of the salts in sea-water, in comparison with those of Forchhammer: —

	Per hundred parts of total salts.	Per hundred of halogen calculated as chlorine.	
	Dittmar.	Dittmar.	Forchhammer.
Chlorine	55.2920	99.8480	Not determined.
Bromine	0.1884	0.3402	Not determined.
Sulphuric acid (SO_3)	6.4100	11.5760	11.88
Carbonic acid (CO_2)	0.1520	0.2742	Not determined.
Lime (CaO)	1.6760	3.0260	2.93
Magnesia (MgO)	6.2090	11.2120	11.03
Potash (K_2O)	1.3320	2.4050	1.93
Soda (Na_2O)	41.2340	74.4620	Not determined.
(Basic oxygen, equivalent to halogens)	(−12.4930)	–	–
Total salts	100.0000	180.5840	181.10

Or, combining acids and bases arbitrarily,

Chloride of sodium	77.758
Chloride of magnesium	10.878
Sulphate of magnesium	4.737
Sulphate of lime	3.600
Sulphate of potash	2.465
Bromide of magnesium	0.217
Carbonate of lime	0.345
Total salts	100.000

The difference between surface and intermediate waters in the contents of lime was 0.0125

Report of the scientific results of the voyage of H. M. S. Challenger during the years 1873-76. Physics and chemistry. Vol. i. London, *Government*, 1884. 307 p., 278 pl., map. 4°.

parts, and that between surface and bottom waters 0.0132 parts, referred to a hundred parts of halogen. The fact that deeper waters do contain more lime than surface-waters, Professor Dittmar attributes to the action of life near the surface in removing lime from solution, and to the tendency of bottom-waters to take it up from the ocean-floor.

As is natural, the alkalinity, too, increases with depth; and the difference between surface and bottom waters in this respect corresponded in Professor Dittmar's determination to 0.014 of lime, which is so near to the figures found in the direct determination of the lime, that the closeness of agreement must be accidental.

Concerning carbonic acid in sea-water, the evidence goes to show, that, as a rule, it is present in insufficient amount to convert to bicarbonate that base which is in excess of the sulphuric acid and halogen, and is free only exceptionally; that in surface-waters it varies inversely with the temperature, and for equal ranges of temperature seems more abundant in the waters of the Atlantic than in those of the Pacific Ocean. The quantities of oxygen and nitrogen absorbed by sea-water are functions of the temperature. Nitrogen varies within the same limits in deep and shallow waters; oxygen is generally present to a smaller extent than the hypothesis of surface absorption of atmospheric air, at the temperature corresponding to the amount of nitrogen found, would demand; and the absolute amount of oxygen in waters of great depths, and occasionally in waters of only moderate depths, is often exceedingly small.

Professor Dittmar discusses his analyses with great elaboration, and devotes much space to chapters upon the salinity and specific gravity, bromine, carbonic acid, alkalinity and absorbed gases of ocean-water. In the analysis the desirability of preciseness was constantly in view. Thus, for example, much stress is laid on the necessity of *weighing* portions for analysis, as is usual with concentrated mineral waters; and, in the estimation of total halogen by Volhard's method, Professor Dittmar secures greater accuracy by *weighing* the precipitating solution of silver nitrate, and then effecting the final titrations with centesimal solutions of ammonium sulphocyanate and silver nitrate. It is quite plain, however, and much to be regretted, that the lack of water at Professor Dittmar's disposal (never exceeding, and often falling short of, two litres) has affected the value of the work. Very few processes of analysis can bear the magnifying of inherent error a hundredfold; and 10 cm^3 of sea-water, to which Professor Dittmar felt restricted for single determinations of total halogen, is an exceedingly small portion when the result is to be expressed in grams to the litre of water, or in parts to the hundred grams of total salts. With an adequate quantity of material at hand, 40 cm^3 need not have been made to serve for a determination of lime and magnesia; nor would such processes as the estimation of magnesia as pyrophosphate, and sulphuric acid as barium sulphate, have been denied ordinary care to insure the purity of the substance weighed. In the case of the lime, it was found, when some of the residues of analysis were combined and tested, that the average error amounted in one set of thirty determinations to eight per cent, and in another series of twenty-six to nine per cent, of the total. With so large a margin of error, the application of the mean correction to individual determinations, as well as to the determinations of a series of twenty-one, the residues of which were not available for examination, is fraught with too much uncertainty. The difference, for example, between the corrections of eight per cent and nine per cent, would amount to nearly three times the difference which Professor Dittmar finds between surface and bottom waters as regards their contents of lime. Fortunately, Professor Dittmar's interesting conclusion concerning the distribution of lime in ocean-water does not rest upon the individual determinations alone, but depends upon his results with the mixtures of 'surface,' 'intermediate,' and 'deep-sea' waters, which allowed him ten times the material for an analysis which he had previously employed, and permitted the adoption of proper precautions.

Professor Dittmar's report closes with some very pertinent suggestions as to future work.

Part ii. contains Mr. J. Y. Buchanan's record of something like fifteen hundred hydrometric determinations of the specific gravity of waters from various parts and different depths of the ocean, and several plates illustrating the variation of density over the surface and in depth. It appears that the waters of the open ocean vary in density between the limits 1.02780 and 1.02400, pure water at 4° C. being taken as the standard.

In part iii. Staff-Commander Tizard tabulates the deep-sea temperatures, and shows, by the method of co-ordinates, the manner in which temperature varies with depth for each station of observation. Tables summarizing the observations, grouping and averaging them by localities, are appended.

The discussion of the records of part ii. and part iii., together with the meteorological data of the expedition, is in course of preparation by Professor Tait and Mr. Buchan.

PUBLICATIONS OF THE NAUTICAL ALMANAC OFFICE.

IN the first part of this volume, Professor Newcomb presents a detailed development of the perturbative function which is applicable to all cases, except extreme ones, in which a general development of planetary inequalities in terms of the time is sought, and by which any required derivatives of the function may be found with great facility. In order to afford some idea of its range of application, he compares this development with others having the same general object; viz., those of Laplace, De Pontécoulant, Peirce, Leverrier, Hansen, and Cauchy. The method of this development has previously been indicated by Professor Newcomb, in the *American journal of mathematics*, vol. iii. The second part of this volume of the 'Astronomical papers' (pp. 201–344) is a determination of those inequalities of the moon's motion which are produced by the figure of the earth, and is by Dr. G. W. Hill, assistant in the office of the *Nautical almanac.*

In Delaunay's 'Théorie du mouvement de la lune,' the perturbations of the moon by the sun were fully treated; but subordinate portions of the theory were in some cases unfinished, and in others untouched. Having waited more than ten years for the promised filling of these gaps by French astronomers, Mr. Hill has in this paper taken up, in his masterful way, the discussion of the perturbations which the moon undergoes on account of the figure of the earth, the appreciable character of which was first brought to light by the analysis of Laplace. In his 'Darlegung der theoretische berechnung,' etc., Hansen has dealt with these inequalities in a very thorough way; but Mr. Hill has investigated these perturbations to the same degree of algebraical approximation that Delaunay adopted in determining the solar perturbations, viz., to terms of the seventh order inclusive; and his memoir is thus most appropriately entitled 'A supplement to Delaunay's theory of the moon's motion.'

The third part of the same volume (pp. 345–371), by Professor Newcomb, treats of the motion of Hyperion. In several papers published during the past five years, Professor Asaph Hall has shown a remarkable retrograde motion in the peri-Saturnium of its orbit, the period of its revolution being about eighteen years. At first sight, this result appears inconsistent with the law of gravitation; for it is easily shown that in the case of a body moving in an eccentric orbit, and disturbed by another moving in a nearly circular one, the secular motion of the peri-centre will always be direct. As Titan is much the brightest, and much the nearest to Hyperion, of all the satellites of Saturn, Professor Newcomb investigates the results of its attraction upon this satellite, and shows that the ordinary theory of secular variations is entirely inapplicable to the mutual action of these satellites, and that we have here an entirely new case in celestial mechanics. The ordinary theory of secular variations presupposes that the mean motions of any two bodies to which it is applied are incommensurable; so that to any given mean longitude of the one, will correspond, in the course of time, every mean longitude of the other. The conjunctions of the two bodies will thus be scattered through every part of the orbit. But four times the mean motion of Hyperion is nearly equal to three times that of Titan; so that, if the two satellites are in conjunction at a given time, when Hyperion has completed three revolutions, Titan will have completed four, and another conjunction will occur at very nearly the same point. In its outer form, this relation between the two satellites is somewhat analogous to that among the satellites of Jupiter; but it is quite different in its cause. Professor Newcomb develops the modified formulae applicable to this case; and among other results of interest is the determination of the mass of Titan equal to $\frac{1}{12500}$ part that of Saturn.

Astronomical papers prepared for the use of the American ephemeris. Vol. iii. parts i.–iii. Washington, *Government,* 1884. 371 p. 8°.

FORCHHEIMER'S TUNNEL-BUILDING IN ENGLAND.

DR. FORCHHEIMER visited England in the spring of 1883, by ministerial authority, to inspect and report upon the class of engineering work represented by the title below, confining himself, for the most part, to tunnels in progress or recently completed. Several most instructive examples are to be seen there, and

Englische tunnelbauten bei untergrundbahnen, sowie unter flüssen und meeresarmen: ein reisebericht. Von Dr. PHILIPP FORCHHEIMER, ingenieur, privatdocent an der königl. technischen hochschule zu Aachen. Aachen, *Mayer,* 1884. 8 + 69 p., 14 pl. 8°.

engineers of other countries can learn much from their study.

He first describes and illustrates the method of constructing the portion of the London underground railway between Aldgate station and the Mansion house, by the way of the Tower. The difficulties encountered from gas and water pipes, sewers, and foundations of buildings, and the necessity of providing for the continuance of street-traffic, called for ingenious contrivances, by means of which the construction was successfully carried forward. Beton or concrete was used for the invert, beton or brick for the side-walls, and brick arches covered the top. All varied in thickness to suit the circumstances of the case, and the superincumbent load.

Next follows an account of the building of a tunnel in London for the Midland railway, with illustrations of the timbering employed in the work, and the tunnel cross-section found best adapted to resist the pressure of the London clay. A brief description of a contemplated subway under the Thames at Woolwich is then given.

The tunnel under the Mersey, between Birkenhead and Liverpool, a little less than a mile long, communication between the ends of which was opened early in 1884; and the Severn tunnel, not far from Bristol, to be four miles and a half in length, and now well advanced, — occupy in description about one-half of this report. The drainage-tunnel below the main tunnel under the Mersey; the arrangements for pumping and ventilation; the introduction of Col. Beaumont's machine, which had previously bored five thousand linear yards through chalk in the proposed tunnel under the English Channel, and here bores a hole seven feet in diameter through the sandstone rock, — are well described. The Severn tunnel is prosecuted with drills driven by compressed air. Progress has been hindered from time to time by the influx of water, even to the extent of completely flooding the works. The pumps required are consequently very powerful, having a capacity of eighty-two thousand six hundred cubic metres in twenty-four hours.

With the exception of two pages devoted to an intercepting or trunk sewer at Brighton, the closing pages are devoted to an account of the examinations and investigations already made in regard to a tunnel under the English Channel, between Dover and Calais, the present state of the project, and the possibilities of the scheme.

The book is handsomely printed, and the illustrations are very clear and explicit.

NOTES AND NEWS.

IN a lecture at Johns Hopkins on the place of the science of hygiene in a liberal education, Dr. Billings states the objections to the establishment of such a course, as follows: first, that there is no existing demand on the part of students for it; second, that the subject is not yet on a scientific basis; third, that the present courses of instruction given in the chemical, physical, and biological departments of the university, include all that a well-educated man need know of this subject, unless he proposes to make it a specialty; fourth, that the students have no time for any studies additional to the course already supplied. To the first objection Dr. Billings replied, that the same might be said as to other branches of the curriculum, — that the majority of students do not know what they ought to study, — and that the question is, whether the time has not come to create the demand, and for the university to lead the way in the matter. The second objection is only partly true. The general rule holds good in man, as it does in the laboratory, that like causes, under like circumstances, will produce like effects. When it has been shown in a number of well-marked cases that polluted water has been the means of spreading typhoid-fever, that overcrowding and foul air precede epidemic typhus, that scarlet-fever or diphtheria has been conveyed to a village by infected clothing from a distance, we have enough information to enable us to advise in similar cases, although we also know that men have drunk sewage with impunity, and that unprotected children have slept in the same bed with a scarlet-fever case and have not taken the disease.

— The foundations under the stone piers supporting the iron bridge, twenty-five feet above low-water level, by which the Wabash, St. Louis, and Pacific railway crosses the Kankakee River, have lately been giving trouble. The bed-rock of shale is hard and soft in places in the short space of a few feet. The three piers were built when the water was high, and were placed on platforms of four thicknesses of pine timber twelve inches square. Before these platforms were located, some of the loose material was removed; but it would appear that the foundation was dug deepest in the centre, and the rapid current of high water washed under and disturbed the piers. In order to fill the space, give a firm bearing over all the bottom, make the piers thoroughly durable, and at the same time not interrupt or interfere with the traffic over the bridge, the application of wooden wedges was suggested and carried out by P. E. Falcon of Chicago. By a strong jet of water and other appliances, the sediment and loose material were cleared away by divers from under two timbers at a time, and the bed-rock was cut away to a level. Oak timbers were fitted to the cavity; and a double row of broad oak wedges, to insure a complete bearing from the middle of the pier to the outside edge, was driven between the oak timbers and the pine platform by means of a steel bar weighing eight hundred pounds, suspended from the bridge by wires, and adjusted to

strike the heads of the wedges. The wedges, when all in place, were driven in the proper order to bring the pier back to its original position, and were then fastened by iron spikes driven by a ram-rod dropped through a gas-pipe as a guide. The work was done on the three piers by three divers in ninety days, and three hundred wedges were used.

— On an obscure passage in the Koran, Mr. W. T. Lynn, late of the Royal observatory, Greenwich, writes as follows: "In reference to Sir George Airy's letter in the *Athenaeum*, suggesting that the famous passage in the fifty-fourth Sura of the Koran does not relate to any phenomenal or supposed miraculous appearance in the moon, but to the ordinary semi-lunar phase when she is said, in the language of astronomers, to be dichotomized, perhaps I may quote Mr. Rodwell's rendering of the passage: 'The hour hath approached, and the moon hath been cleft. But if *the unbelievers* see a miracle, they turn aside and say, "magic that shall pass away." And they treat *the prophets* as impostors, and follow their own lusts; but every thing is unalterably fixed.'

"This hardly reads like a reference to an ordinary appearance of the moon as a chronological datum. The 'unbelievers' could surely not speak of that which occurs every fortnight as 'magic;' though many might conclude from previous experience that a peculiar appearance, produced by some meteorological condition, even though of a more remarkable kind than they had seen before, would pass away, and had no prophetical meaning. As to the expression, 'every thing is unalterably fixed,' Mohammed would probably mean that even miracles took place, like ordinary phenomena, by divine appointment. Mr. Rodwell, like Sale, thinks the word translated 'hath been cleft' may mean 'will be cleft,' the future 'being expressed by the prophetic preterite, and the reference being to one of the signs of the last day.' Nevertheless, he admits that the passage may refer to a miracle said to have been wrought by Mohammed; and this is, I believe, the general impression of Mohammedans with regard to it. I well remember travelling many years ago to Oxford with an Egyptian who had some scientific acquaintance with astronomy, and was at the time visiting the English observatories; and, on my remarking that Mohammed laid no claim to miraculous powers, he exclaimed, 'Oh, pardon! il a fait des miracles; il divisa la lune en deux parties, et puis' — Here my companion broke off his own sentence with a hearty laugh, sufficiently indicating his own scepticism of the alleged miracle. He was evidently about to refer to the later accretions of the story with which I was familiar as given by Gibbon from Maracci; but he gave the Koran as his authority, and his primary reference was undoubtedly to the passage quoted by Sir George Airy."

— The Swedish academy of sciences has recently published the results of the measurements of the level of the Baltic, begun in 1750, to decide the controversy on the point between Celsius and the German scientific men of his day. The verdict of these hundred and thirty-four years is that both parties were right, and both were wrong. The Swedish coast has been steadily rising, while that along the southern fringe of the Baltic has been as steadily sinking. The dividing-line, along which no change is perceptible, passes from Sweden to the Schleswig-Holstein coast, over Bornholm and Laland. The northern part of Sweden has risen about seven feet. The rate of elevation gradually declines as we go southward, being only about one foot at the Naze, and nothing at Bornholm, which remains at the same level as in the middle of the last century. An example will best illustrate the process. The cliff near Pieta, known as 'Stora Reppen,' was, in 1851, ninety-three centimetres higher above the water-level than it was in the year 1750; and on the 12th of August, 1884, it was found to be about fifty centimetres higher than in 1851, showing that the rate of elevation had been quickened during the thirty years immediately preceding. The general average result would be that the Swedish coast has risen about a hundred and forty-three centimetres (nearly fifty-six inches) during the last hundred and thirty-four years.

— Writing to *Kosmos* from the Brazilian province of Rio Grande do Sul, Dr. H. von Ihreng, in regard to a case of polydactylism in a horse, which came under his own observation, says he has scarcely spoken to any one, who has travelled much in that region, who has not himself met one or more cases of the kind. The extra toes are on the inner side of the fore-feet. The question, he says, forces itself upon one, whether there has not been a survival of the old race of Equus in a few regions, which has escaped the notice of the discoverers and early settlers of the country. "The horse certainly still existed in the Rio Grande during the pleistocene era, as I have received horse-teeth from alluvial soil which were found in digging a well, and which agree in the very slightest details with the corresponding teeth of Equus Caballus. It is possible that among the wild horses of South America there are still to be found descendants of the native horses of the alluvial."

— Human skulls and other bones were lately dug up from the kjökkenmöddings at Muyem, near the Tajo, Portugal, which, judging from the character of the deposits and accompanying fauna, can almost with certainty be ascribed to the quaternary epoch. The earlier race was dolichocephalic. To this belonged a number of skulls of wonderful uniformity, offering so few differences, except of a sexual character, that we have unquestionably to do here with a homogeneous race. The prognathism of the skulls, and the length of the fore-arm, such as is only met with among negroes, recall at once the African races; while the capacity of the cranium is so small that it can be compared only with that of the Australian. There are also but few races of so small stature as these old inhabitants of Portugal. Only three brachycephalic skulls were found; and, judging from the organic marks, these belonged to a larger race than the dolichocephalic.